August 29, 1951.

Dear Bernie:

A group of cultures should arrive shortly. Their labels should be self-explanatory.

Pseudomonas fluorescens is a very attractive organism to work with. The penicillin method works almost as well as with K-12 (which is by no means spectacular); in combination with replica-platings, there should be no difficulty whatever in securing a large stock of mutants. The media used for E. coli will do very nicely, except that P. fluorescens is rather more aerobic, and should be shaken or aerated routinely. Also, its temperature optimum is close to 30 C. (I spoke too soon about our summer weather— this is our room temperature today!) I have not yet seen any non-fluorescent mutants, so that this makes a convenient marker against contamination. With some mutants derived from it, I am also including Roger Stanier's strain A3.12, with which he did much of his work on aromatic oxidation. See J. Bact. 55: 477 for its general metabolic behavior.

If I can resuscitate them, there may also be some mutants of B. subtilis Marburg, but on second thought you might better get these directly from Burkholder at Yale OBL.

In my last letter, I forgot to mention that "Papers in Microbial Genetics" is in press, and it would be rather drastic to make any changes or additions now. I think you will have nothing to apologize for, however, with respect to the revision of the aromatic scheme. At any rate, a) the main purpose of its inclusion was to illustrate the methodology, not indoctrinate the details, and b) Your name is signed to the original publication, to your credit and responsibility. The amendations are not so drastic, as far as I can see, as to justify any special comment about it. However, when your papers come out, I expect to cite them to the class as the further flowering of biochemical genetics of bacteria.

The "Replica Plating and Indirect Selection of Bacterial Mutants" is being sent out today, to J. Bact. We would probably never have bothered about it if Eagle hadn't made such an impression; I trust his stimulus may continue to be useful, and I must confess that I can see no objection to including physiological (nonheritable) adaptations among the processes of resistance, especially when, for example, penicillinase behaves so remarkably as an adaptive enzyme in B. cereus. But I think you're right: although Eagle takes a conservative stand to geneticists, he probably would like very much to chuck out spontaneous mutation altogether. Werner was supposed to come through here about this time, but hasn't shown up so far. If you still want to make the sulfa-resistant mutants, I'll be glad to try them in heterozygotes.

What did you think of these two wonderful papers in the July J. BAct.:pp 53, [redacted] and p.747 of the June issue